Response to reviewer comment 2 (RC2):

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General response

We thank the anonymous Referee for the constructive comments regarding the cosmogenic nuclide application, which help us to improve the quality and clarity of our work significantly. We address the main concerns in the light of the comments by referee #1, who suggested to restructure the manuscript and to focus on the pre-conditions leading to rock fall processes. This is a recommendation, which we follow (see also response to reviewer comment 1; RC1). As a consequence, we move the cosmogenic nuclide part to the new Appendix A. This allows us to address the 2 main concerns of reviewer #2:

1) The methodological concerns (see detailed responses below): "*One concern is that there is little new data offered here and what is presented is close to the limits of what might be considered acceptable in terms of noise-to-signal. […]*" We concede that the measured ^{10}Be concentrations are low and close to the detection limit. Thus, we agree that it is essential to assess the blank correction (see detailed responses below). Accordingly, we present our arguments for using the long-term variance weighted average blank correction, for which we provide statistics on its variability (Table 1). However, we also provide the results of the in-batch blank measurements, which is approximately 2x higher than the weighted long-term average value. We concede that using the higher value for blank correction, several samples would not show acceptable signal-to-noise ratios in 3 samples, with the consequence that that these ^{10}Be concentrations would not be interpretable. We discuss these points in Appendix A, mark the denudation rate value for EW-01 in the main manuscript as potentially non-interpretable (due to ^{10}Be concentrations at the detection limit), and point to the discussion in Appendix A. However, we think that the ^{10}Be data is worth being reported in the Appendix; especially in the

context for understanding the challenges that are associated with the modelling of the *in situ* denudation rates in such settings (see also detailed response below).

2) The reliance on a previous publication, as referee #2 points out: *"[…] While the paper reads well, necessary information is often lacking to properly assess what is being done and there is too much reliance on a previous publication (Mair et al., 2019), which the reader is essentially forced to read if they want to understand this paper. […]"*.

The new Appendix A provides a concise summary on the denudation rate modelling. Furthermore, we understand that there is a need to clarify why the consideration of inherited ^{10}Be concentrations is important upon modelling. We also realize that it is relevant to discuss the consideration of a model scenario where denudation rates are uniform (see responses below). Both aspects are provided and discussed in Appendix A.

From here on, we will address each point individually and in the same order, as the reviewer raised them.

One concern is that there is little new data offered here and what is presented is close to the limits of what might be considered acceptable in terms of noise-to-signal. This leads me to be unconvinced that what data is presented support the findings. I do not agree with the authors that the relative analytical uncertainties of 11-69% at 1 simga are small (as is claimed in Line 296), instead they are hampering a sound interpretation of a small set of data.

We concede that relative uncertainties of up to 69% at 1 sigma are not small. We now discuss the ^{10}Be data in more depth in the new Appendix A (see also response to related comments below). We clearly point out the limitation of the small data set and the effect of the large uncertainty. We also rewrite the corresponding section 5.1 to comply with the reviewer's comments and the new structure of the entire paper.

Modelling of the limited dataset is valid to try and extend the approach and investigate erosion in a more general sense, but the profile modelling is either missing crucial information, or is inappropriately used. The authors apply a published model (Hidy et al. 2010) that to my knowledge has been mostly used in order to extricate age/erosion information in situations where variable pre-exposure could be a concern. This has been suitable for sedimentary deposits, where samples have a pre-depositional exposure history (inheritance). In the case of bedrock, as sampled here, any inheritance must have other origins. […]

We acknowledge the need for clarification here. As the reviewer correctly points out, inheritance in bedrock samples could not stem from a pre-exposure history. We thus refrain from using the term 'inheritance' to avoid any confusion with the concept established in the cosmogenic community and based on work on sediments. For bedrock profiles, we explain the occurrence of inherited nuclides with a history where bedrock was previously exposed, and our samples were shallow enough to start accumulating cosmogenic nuclides. A scenario, which could have achieved this, would be a mass wasting event that was too small to completely reset the TCN clock. Alternatively, inherited nuclides at depth can build up through a prolonged exposure period during which the surface has experienced a low denudation rate, followed by a period of higher denudation (which translates to the current exposure). Such scenarios would allow for the accumulation of "excess nuclides" at depth. We expand the corresponding section in Appendix A accordingly to explain these mechanisms and to clarify this point (see also the following 2 responses below).

[…] I'm confused as to why the authors consider production by muons to be an inherited component in a study of erosion (e.g. L176). Muon production at depth as the rock erodes is not 'previous exposure', as the authors state, but part of the ongoing exposure that is being used to constrain the erosion rate. […]

There is a misconception here, due to previously ambiguous phrasing in the manuscript. We calculate muogenic production at depth in the generally accepted way (e.g., Balco et al., 2008; Hidy et al., 2010; Marrero et al., 2016). We use the inference that inherited nuclides would only be produced by muogenic production to define a boundary condition for the model (see responses below). In case where inherited nuclides are present, an initial landslide would remove some meters of bedrock, with the consequence that all nuclides from spallogenic production would have mostly been removed. If the rockfall was much larger and removed several tens of meters of bedrock, then nuclides from muogenic production could also have been removed. For the alternative scenario of a prolonged exposure (see previous response above), the jump to higher denudation rates would result in a situation where near-surface bedrock with nuclides from spallation would be removed after the shift towards high denudation rates. Thus, we can use the site-specific muon attenuation length as parameter to model the contribution of inherited ^{10}Be in samples collected at greater depth, and we can compare these concentrations with those from surface samples. We elaborate a corresponding statement in Appendix A together with the underlying assumption and a justification thereof.

[…] Assuming inheritance values equivocal to the concentration of the surface sample and relating this to muons (L168 and L180) would seem to suppose a large enough landslide occurred to entirely remove the spallogenic component. This, however, would go against what is claimed on L305, that the 'inheritance' is too large to support the notion of a deep landslide, and instead they mention multiple dm thick rockfall events (see also below on this point). […]

The statement in question describes the boundary conditions for the model setup. We consider an uppermost limit in our model where in the surface sample the inherited nuclides make up 100% of the total TCN concentration. This would correspond to a large and recent rockfall event, and the contribution of inheritance with depth should then follow the muon attenuation curve. This is the case because until the rockfall event, the rocks would have resided at depths where only muogenic production occurs (see also previous response). We rephrased the mentioned passages for clarification purposes.

[…] On L300 it is mentioned that the inherited component likely comes from greater exposure at depth, before the current exposure period. Unless the authors are arguing for some kind of intervening burial, which I'm pretty sure is not the case, these would not be different periods of exposure, but one period perhaps separated by a hiatus (i.e. non-steady-state erosion). […]

The reviewer raises a point here which we have not carefully addressed yet, but which we will do in the revised manuscript. The inherited nuclides could potentially stem from a rock fall event prior to the current exposure history, or from a change in the denudation rate (see previous responses). The latter would correspond to the scenario in discussion here. However, it would represent a shift from a mechanism where continuous erosion occurs first at low rates and then at higher rates. We clarify the statement in question in the revised manuscript

[…] If non-steady-state erosion is the case it would go against a model that tries to fit a smooth production profile with depth; though erosion via stochastic mass wasting would arguably better explain why there is difficulty fitting a smooth profile through the data than some notion of inheritance.

The reviewer touches a point, which we will certainly consider. One assumption of the model is that erosion occurs in a uniform and steady way (Lal, 1991; Hidy et al., 2010). We have to assume that small-scale stochastic erosion events over a very short timescale (< 1 yr) could correspond to a continuous erosional mechanism if longer time scales are considered (> 10 yr). The validity of this assumption can to some extend be tested by comparing the modelling results with the ^{10}Be concentrations in the depth profile, where reasonably low reduced chi square values can be interpreted as indication of a smooth profile. However, we adapt the text in Appendix A to explicitly state and discuss these assumptions.

[…] Perhaps I'm missing something fundamental here but if so the authors need to do a better job of explaining why they are including inheritance in the first place, and then why they are relating it to muogenic production.

We seize the opportunity to expand on the cosmogenic nuclide method part in the Appendix to clarify the unclear points raised by the reviewer (see responses above).

The authors claim dm sized rockfall erosion. I suspect with dm size erosion events one could sample a metre or so away and get different results (i.e. these blocks fall from a specific site stochastically). Whether this is an issue depends on what is meant by dm; 10cm, 90cm? I don't see the support for this claim of erosion thickness other than the jointing would suggest it. […]

We infer that the occurrence of rock falls at the scale between 1 cm and 10 cm, averaging on a temporal scale of > 10 yrs, could be considered as a steady state denudation scenario, which we employ for TCN applications (see related response above).

[…] That is, the bedrock structure data would be better used as a parameter constraining possible mass loss depth in an erosion rate modelling exercise, rather than being an assumed outcome of the cosmo profile analysis that it would likely be causing trouble for anyway (see above RE fitting a smooth profile to stochastic erosion events). Approximate fits to the data can be gotten by assuming the simplest case of a large rockslide 2.2 kyr ago setting surface concentrations to zero. Admittedly the fit is not as good as shown by the authors as I use a much simplified approach but my point is the claims based on the cosmo data are weak (non-unique outcomes are clearly possible), not fully explained and are specific to certain sites.

We thank the reviewer for the suggestion, but we refrain from such an approach, as it would require several assumptions, as pointed out by the reviewer. In particular, this infers that the rockfall event would have to be large enough to remove all previous nuclide concentrations, which is contradicted by the "truncated" shape of the TCN depth profiles (Mair et al., 2019). The underlying assumptions for the interpretation presented in this work (and also in Mair et al., 2019) are justified as outlined above. Furthermore, the model would allow us to actually test such a hypothesis. It should return a result with an inheritance close to zero, a low denudation rate and a minimum age close to 2.2 kyr.

The results are sensitive to the blank correction due to the low 10Be concentrations. Blank corrections as high as 19% could be acceptable if the authors can show the subtraction is robust. This probably *requires several in-batch blanks, rather than a longterm lab background average, which needs to be justified here. The vague nature of the blank subtraction as it's reported lessens the confidence in such low concentration data, i.e. what is the uncertainty in this long-term blank value (not given on L140 or in table 3); was a blank/s measured in the batch, or at the same time in the lab, and if so what of the results? If the authors are forced to use a long-term average as no in-batch measurements were made I would expect to see some discussion of how variable this value has been over time (long-term averages would mask occasionally high/low values which is a problem when it comes to measurements close to the lab background). Is there any idea of what inter-batch background variability is?*

We thank for bringing this point up. We actually use the long-term, variance-weighted average blank value of 2.44 x 10^{-15}, which is calculated from several in-batch blanks for each bottle of Be spike (Table 1). However, the long-term, variance-weighted average ratio should be 2.48 x 10^{-15} with an uncertainty of 18.8 % that is based on 28 blank measurements (see Table 1 for all measured in-batch blank values from the corresponding Be spike batch). We apologize for initially reporting an incorrect value and for erroneously calling it 'long-term average' instead of 'long-term, variance-weighted average', which makes a difference. We justify the use of the 'long-term, variance-weighted average' because the main contribution of contamination is likely to stem from the impurity of the used carrier (Scharlau Beryllium standard solution 1000 mg/l BE03450100 by Scharlab S.L.). We justify this by having established high standard clean lab protocol, e.g., by using only supra-pure acids for dissolving, which in general leads to very stable and low blank ratios, even across several spike batches (we are happy to provide more data here, if needed). However, the in-batch measured blank ratio for the EW-01 samples is 4.81 x 10^{-15}, almost 2x times higher than the long-term, variance-weighted average ratio. Using this value for blank subtraction would amount to a 29 - 35% relative correction for samples EW-01-4,-5,-6, a level at which we would not consider the measured concentrations as much different to the blank. Hence, we agree that this needs a transparent discussion, which we now provide in Appendix A. We indicate the result of EW-01 in the main manuscript as potentially non-interpretable, due to the low concentrations at the detection limit, and we refer to the Appendix.

Line by line responses

L61- *The way this is written makes it appear as though new 36Cl data will be presented, rather than the inclusion of previously published data in the discussion. Same goes for the conclusion section L433.*

Clarified.

L64- *Saying the long-term denudation of the mountain will be quantified sounds a bit too grand and is incorrect, as the rates reported are pretty short term and for a few specific locations only.*

We recognize that there is a difference in the definition of long-term between the rockfall and cosmogenic nuclide community. We clarify it by relating it to the millennial timescale.

L100- *Some discussion of the issues that might relate to sampling a constructed tunnel would be appropriate. How pristine were the surfaces sampled, especially for the zero depth sample, was it near the lip of the tunnel?*

We provide now a brief description of how and were we collected the samples, and we indicate that the zero depth sample was taken at the present bedrock surface.

L158- *The shielding correction is high (0.55), so sensitivity of the results to the exponent used in the topographic shielding correction ('m' in Dunne et al 1999) should be considered.*

We use a coefficient of m = 2.3 ± 0.5 for the angular flux dependence, following Nishiizumi et al. (1989). In a general case, a variation in the exponent m would have only a small effect on the shielding factor as the dependence on the angular flux varies only slightly (Gosse and Phillips, 2001; Fig 5). The shielding is commonly defined as ratio between open sky flux and blocked out flux (Dunne et al. 1999; Gosse and Phillips, 2001), thus flux variations from changes in m should amount to a ~ 5% difference in shielding factor and/or attenuation length for values between 1.8 and 3.5 (Heidbreder et al., 1971), depending on the parametrization. This would cause a corresponding increase/decrease in the absolute exposure age. The denudation rate values would vary by a few percent only, but the overall results would not systematically change.

L168- *If the maximum likely age is 20 kyr why then use 75 kyr?*

We select a broad range of model constraint values in an effort not to predetermine the solution space and thus not to bias the interpretation. We particularly test if, hypothetically, the sites were above or below the LGM glaciation.

L207- *Applying values that are 'slightly higher' is vague and seems arbitrary.*

Specified and justified.

L298- *The 'clear minimum' for denudation in the different simulations is zero. I'm not sure this suggests a clear minimum, or a problem, as it implies the model wants to go below zero. […]*

The clear minimum refers to the reduced chi square space, which coincides with the mean and median denudation rates and thereby indicating a Gaussian distribution of the denudation rate histogram. We clarify the text accordingly.

[…] I also see no justification for using these 3 values?

We think that the reviewer refers to the total allowed denudation values of 12, 15 and 20 m. These values are used as constraints for our model to work. We try to realistically estimate the maximum amount of removable bedrock during the exposure, and run three setups to test the independence of the result from this boundary condition. The values are obtained following these arguments:

We use these 3 values because the deepest samples were taken at depths close to or exceeding 3 m and consequently, the production of TCN has almost exclusively occurred by muon pathways. Muon attenuation scales exponentially, with reported muon attenuation lengths between ~4000 and 5300 ± 950 g cm^{-2} for 2.7 g cm^{-3} rock density (e.g., Braucher et al., 2013). This translates to muon attenuation depths of ~15 m to ~19 m for 1 attenuation length, and ~30 to ~38 m for 2 attenuation lengths, which accounts for a reduction of muogenic production by ~63% and ~87%, respectively. This means that independent of the attenuation length, our deepest samples would have been located at a depth of > 23 m at the start of the exposure to allow for more than 20 m of total erosion to occur (Mair et al., 2019). Any potential nuclides inherited from before would then have accumulated at this depth or even deeper at muon production rates < 2% of the surface production rate. These are the major arguments why we run 3 values ≤ 20m. We add a short justification in the Appendix.

L301- *I don't understand how the uniformity of the 'cut-off' depths suggests a robust measurement. Time simply extends by a proportional amount to allow for the greater amount of denudation (i.e. Table 4)?*

We relate this question to a misunderstanding. The agreement of the modelled denudation rates show that the results are independent on the selection of a maximum for the total amount of denudation. We clarify the Appendix text accordingly.

L311- *This statement probably needs to cite the Mair et al 2019 study.*

Referenced now.

L304- *Define 'large' inheritance? The deepest sample is within zero at 2 sigma. I don't think these arguments about concentrations at depth are sound for such large uncertainties. Also, 'lower' should be 'higher', or the statement needs to be written more clearly.*

We rewrote the statement to focus on the shape of the depth profiles and correct for 'lower' to 'higher'.

L315 and L411- *If the argument is being made for steady-state erosion (though what steady-state means in relation to dm size chunks is unclear) the rate should persist for several multiples of the attenuation length (see the Lal 1991 paper cited). I'm not sure if it's appropriate to talk about the minimum age, based on assuming the sample concentrations represent exposure ages measurements, as being the time over which the measurements are appropriate. This point needs more explanation.*

We suggest an erosion mechanism at a scale between 1 cm and 10 cm, which occurs steadily over a temporal scale of < 10 yrs. This can be considered as a steady state denudation mechanism if TCN timescales are used as reference (see responses above). We further clarify that the reported minimum ages are the modelled minimum ages. This also accounts for the occurrence of inherited nuclides. Accordingly, we do not directly relate concentrations to exposure ages. The minimum ages refer to a minimum time span during which the modelled conditions are applicable (i.e., denudation scenario, nuclide production etc.).

Fig 1A could be the same orientation as the diagrams (i.e. it's currently a mirror image of 1B).

Changed accordingly.

L155/L158- *What are spallogenic particles?*

Corrected to spallogenic production.

#	10Be/9Be ratio	Rel. err. [%]	Ratio err.
1	9.00E-15	34.90	3.1410E-15
2	2.40E-15	82.90	1.9896E-15
3	2.40E-15	90.60	2.1744E-15
4	2.30E-15	103.20	2.3736E-15
5	9.50E-15	35.30	3.3535E-15
6	1.20E-15	180.10	2.1612E-15
7	1.30E-15	180.10	2.3413E-15
8	1.57E-14	23.20	3.6424E-15
9	5.84E-15	27.85	1.6259E-15
10	5.31E-15	20.04	1.0632E-15
11	4.70E-15	23.60	1.1104E-15
12	1.21E-15	65.13	7.8934E-16

13	2.35E-15	50.01	1.1745E-15
14	8.42E-15	18.94	1.5947E-15
15	8.51E-16	57.77	4.9154E-16
16	1.31E-14	15.08	1.9771E-15
17	1.17E-15	33.37	3.8897E-16
18	6.48E-15	27.76	1.8001E-15
19	5.79E-15	100.09	5.7961E-15
20	2.35E-15	103.91	2.4413E-15
21	5.75E-15	22.98	1.3222E-15
22	2.22E-15	29.54	6.5612E-16
23	4.81E-15	23.23	1.1182E-15
24	2.74E-15	32.04	8.7939E-16
25	4.23E-15	20.44	8.6576E-16
26	5.81E-15	19.44	1.1303E-15
27	1.27E-15	37.18	4.7128E-16
28	3.89E-15	32.60	1.2696E-15
Variance-Weighted Avg. Ratio			**2.478E-15**
Variance of the Var.-Weighted Avg. Ratio			**4.656E-16**
Standard error of the w. avg. Ratio			**1.831E-16**

Table 1. Measured blank ratios used for the long-term, variance-weighted blank correction for the used Be spike batch.

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